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Reply

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In their comment, *Gombosi et al.* [this issue] (hereinafter referred to as GPL99) claim that the conclusions reached by *Raeder* [1999] (hereinafter referred to as R99) are not correct and not supported by their simulations. Specifically, they raise the issues of (1) results from other codes for northward interplanetary magnetic field (IMF), (2) the inherent numerical resistivity of different schemes, (3) mesh convergence, and (4) the diffusion of the bow shock in the high-resistivity cases. Each of these issues warrants clarification.

GPL99 essentially claim that their model is more accurate than anyone else's. This assertion is based on the notion of "mesh convergence." As I shall discuss below, mesh convergence is at most necessary, but not sufficient for validating a simulation. GPL99 do not offer any physical explanation for their results, that is, why there should be a closed magnetosphere with a 50 R_E short tail as a result of a moderately northward IMF. Neither do they show any conclusive experimental evidence to support their assertion. They do, however, present a simulation based on the first-order accurate and highly diffusive Rusanov scheme (as I do in this reply) which shows a closed magnetosphere. These simulations corroborate the main result of R99, namely, that large values of resistivity produce a closed magnetosphere for northward IMF conditions.

1. Other Northward IMF Simulations

GPL99 present a list of global simulation studies of Earth's magnetosphere under northward IMF conditions that were published over the past 15 years. They point out correctly, as I did in my paper, that all of them predict a closed magnetosphere except for *Raeder et al.* [1995] and R99. GPL99 apparently imply that the majority must be right.

GPL99 point out that very different codes and numerical schemes were used in these studies to add further credibility to this conclusion, although later they say that "Models 4-7 ... are based on similar techniques." The latter statement is false, as far as the GPL99 and my model is concerned (I cannot comment on the other models because their numerical details are not published). The GPL99 model [*Powell et al.*, 1999] modifies the MHD equations with $\nabla\cdot\mathbf{B}$ terms, splits the equations by waves (characteristics), uses an approximate Riemann solver to calculate numerical fluxes, and subtracts Earth's dipole to avoid large gradients and spurious current near Earth. I do none of the above in my model, but I use straight flux limited differences and the constrained transport method of *Evans and Hawley*, [1988] which guarantees (as opposed to approximates) $\nabla\cdot\mathbf{B}$=0. Thus there are certainly more differences between models 4 and 7 than implied by GPL99.

Because most studies of GPL99's list do not discuss specifics about their simulations (for example, boundary conditions) in enough detail, it is virtually impossible to discuss the reasons that lead to their results. None of the studies listed in Table 1 of GPL99, except R99, makes any attempt to quantify the inherent diffusivities in their respective codes. Thus I find the argument that many other codes have produced closed magnetospheres for northward IMF misleading and also irrelevant.

2. Numerical Resistivity

GPL99 point out correctly that any discrete numerical scheme to solve the MHD equations will lead to diffusion and that these artificial effects are difficult to quantify. It is an unfortunate reality of computational physics that there is no method to solve hyperbolic equations, such as the ideal MHD equations, numerically without introducing numerical diffusion and dispersion when discontinuities are present in the solution [*Sod*, 1985]. The quest has always been to minimize these effects. While schemes with no diffusion can be constructed (for example, central second-order spatial differences with leap-frog time stepping), they suffer from excessive numerical dispersion which makes them useless for computations that involve shocks and discontinuities. The latter effect leads to overshoots and undershoots in the solution near discontinuities with adverse numerical effects [*Hirsch*, 1990, p. 518]. Thus one strives to develop schemes that are monotone [*Godunov*, 1959], that is, schemes that do not produce artificial extrema. *Godunov* [1959], and later *Harten et al.* [1976] and *Osher* [1983] showed that monotone schemes are at most locally first-order accurate and thus excessively diffusive.

Modern schemes, which come under different names such as “flux-corrected transport” (FCT), “total variance diminishing” (TVD), or “self-adjusting hybrid schemes” [*Boris and Book*, 1973; *Zalesak*, 1979; *van Leer*, 1973, 1974, 1977; *Harten and Zwas*, 1972; *Harten*, 1983, 1984; *Sweby*, 1984; *Yee*, 1985, 1987] try to achieve monotonicity with high global accuracy by blending high order fluxes with low order fluxes, using so-called “flux limiters” or “smoothness monitors.” In essence, these schemes are of high order everywhere, except at discontinuities where they become first order and highly dissipative. The differences between these schemes lie mostly in the computation of the flux limiters, but the ways in which the flux limiters are applied (conserved variables versus characteristics) also provides a distinction. Of the models listed in GPL99's Table 1 only models 4, 5, and 7 use flux-limited schemes (model 6 is unknown). The assertion in GPL99 that my code is based on the Rusanov scheme (which is of first order and very diffusive) is only partially correct. I clearly state [*Raeder*, 1999, p. 17,360]:

> The gasdynamic part of the equations is spatially differenced by using a technique in which fourth-order fluxes are hybridized with first-order (Rusanov) fluxes [*Harten and Zwas*, 1972; *Hirsch*, 1990]. The magnetic induction equation is treated somewhat differently [*Evans and Hawley*, 1988] in order to conserve $\nabla\cdot\mathbf{B}$=0 exactly. The time stepping scheme for all variables consists of a low-order predictor with a time-centered corrector, which is accurate to the second order in time.

The first sentence leaves no ambiguity to the fact that my scheme is flux limited and that Rusanov fluxes are only used as the low-order fluxes, whereas the high-order fluxes are of fourth order. The flux limiter and the hybridization technique are described in detail by *Harten and Zwas* [1972]. The second sentence says that I use the algorithm described by *Evans and Hawley* [1988], which includes the divergence free placement of the field variables as well as the flux computation using the flux limited van Leer scheme [*van Leer*, 1977]. Thus my code is globally of fourth-order spatial accuracy in the gasdynamic variables (density, momentum, and energy density), of second-order spatial accuracy in the magnetic field components, and of second-order accuracy in the time differencing. Consequently, the implication that my code is inherently more diffusive than models 5 and 6 of GPL99's list is wrong.

In order to examine the differences between my code and a pure Rusanov code I have stripped my code of its high-order fluxes and rerun the case presented in R99 with this low-order version of the code. Specifically, the Rusanov flux function [*Zalesak*, 1981] for the gasdynamic variables is given (for one dimension) by

$$f(U)_{i+1/2} = \frac{1}{2}(F_i + F_{i+1}) - \frac{1}{4}(a_i + a_{i+1} + V_i + V_{i+1})(U_{i+1} - U_i),$$

where U is one of the variables density, momentum, or energy density; F is the physical flux; a is the sound speed; V is the velocity; and f is the numerical flux. For Faraday's equation the flux function is given by

$$f(B)_{i+1/2} = \frac{1}{2}(F_i + F_{i+1}) - \frac{1}{2}V_{i+1/2}(B_{i+1} - B_i),$$

where B is any of the field components B_x, B_y, or B_z. The latter flux function is somewhat less diffusive than the original Rusanov version which was only intended for the Euler equations. Including the sound speed would not be appropriate here. Note that this flux function reduces to the first-order upwind flux function in certain cases (for example, when the flow is parallel to an axis).

The results are shown in Figure 1. The parameters are all the same as for the case presented in R99, except for the flux functions. No uniform or anomalous resistivity was added. Figure 1 should be compared to Plate 1 of R99. Clearly, the Rusanov model leads to a closed magnetosphere. The length of the tail is $\sim$80 R_E. Comparison with Plate 1 of R99 indicates that the inherent resistivity of the Rusanov model is comparable to a uniform resistivity of η_0=2-3$\times10^5$ Ω m. Obviously, first-order flux functions produce closed magnetospheres.

3. Mesh Convergence

GPL99 raise the issue of mesh convergence. Simply put, increasing the mesh resolution should reduce the numerical errors, and in the limit of zero mesh resolution the numerical solution should converge to the true solution. However, this is only true if the algorithm is at least second-order accurate in space and time. If the discretization errors are of less than second-order, increasing the mesh resolution will have little or no benefit. For example, the local truncation error of a first-order scheme would become smaller with decreasing cell size, but there are also more steps needed for a given time interval because of the Courant-Friedrichs-Levy (CFL) stability condition, with no net gain of global accuracy.

GPL99 provide a mesh-convergence study of their own code in which they decrease the cell size by a factor of 8 from the coarsest to the finest grid. There are essentially no differences in the solutions which they take as conclusive evidence that their solutions are free of numerical effects. There are four points to be made here.

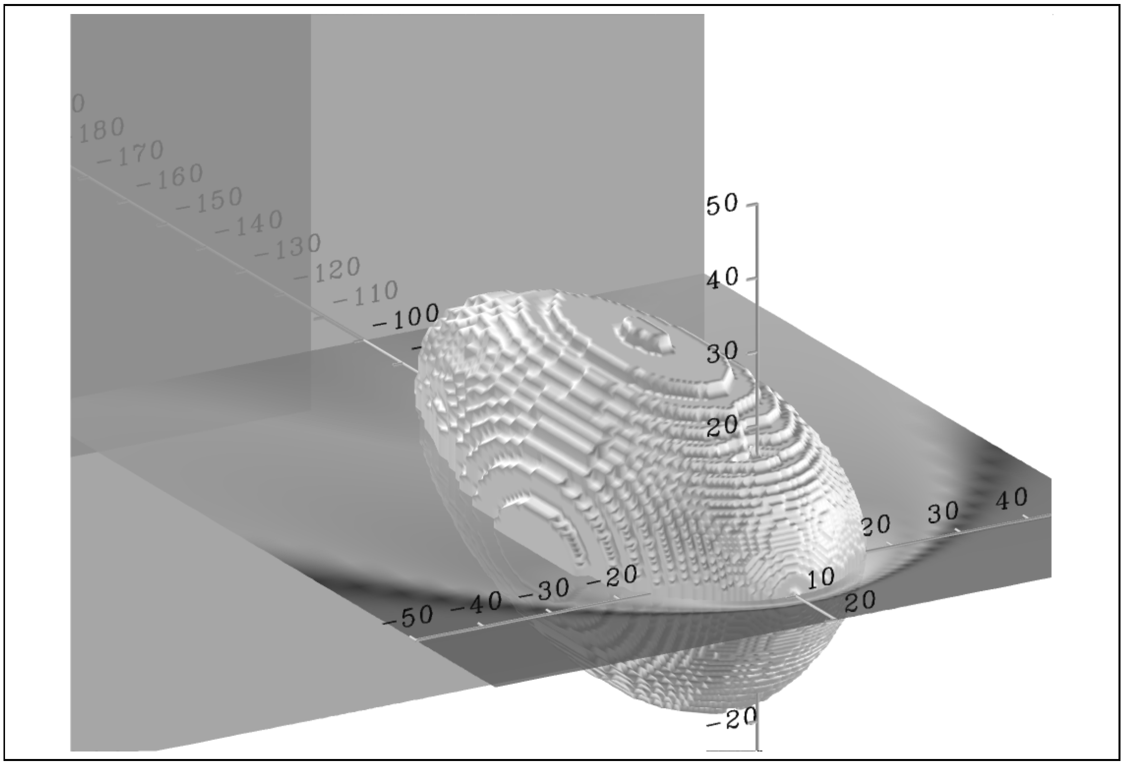

Figure 1. Rendering of the magnetosphere, showing a model run with exactly the same parameters as in R99, except that the first-order Rusanov scheme was used as the flux function, and with no anomalous or uniform resistivity.

First, mesh convergence is a necessary but not a sufficient criterion. If the algorithm is not at least of second-order accuracy in space and time, the convergence test will show nothing or almost nothing. Although GPL99 claim that their code is locally of second-order accuracy, there are still various ways by which other errors can be generated. For example, insufficiently accurate grid restriction and prolongation in the adaptive mesh code, the approximate Riemann solver, or a too conservative flux limiter can all produce additional errors. Also, errors generated by the nonconservation of $\nabla\cdot\mathbf{B}$ may be producing an effective resistivity. Only models 4 and 7 guarantee a truly divergence-free magnetic field solution by employing the constrained transport algorithm of *Evans and Hawley* [1988], while all others use approximations or do not care. Model 5 (GPL99) is particularly worrisome, as shown by *Powell et al.* [1999, p. 303], who state: "The bad news here is that, ..., $\nabla\cdot\mathbf{B}$ itself is constant with grid refinement." and [*Powell et al.*, 1999, page 290] "Thus, for a solution of this system, the quantity $\nabla\cdot\mathbf{B}/\rho$ is constant along particle paths and therefore, since the initial and boundary conditions satisfy $\nabla\cdot\mathbf{B}$=0, the same will be true for all later times throughout the flow. The only ambiguity arises in regions which are cut off from the boundary." The latter statement is not quite true because $\nabla\cdot\mathbf{B}$ errors also arise from spatial discretization, which is clearly shown by *Powell et al.* [1999, Figure 5]. Of course, the detailed effect of $\nabla\cdot\mathbf{B}$ is not known, but it may well affect the solutions in a similar way as resistivity.

Second, the mesh-convergence test by GPL99 clearly shows that no convergence toward the true mathematical solution has occurred. GPL99 employ the ideal MHD Ohm's law, i.e., $\mathbf{E} = -\mathbf{v}\times\mathbf{B}$. Because this Ohm's law does not allow for a parallel electric field, by resistivity or otherwise, no magnetic reconnection should occur. However, even their results with the highest spatial resolution are characterized by reconnection between IMF and lobe field tailward of the cusps. Thus, either their algorithm has converged to the wrong solution, or convergence has not occurred. GPL99 assert that the argument of nonconvergence is irrelevant since any ideal MHD code would have numerical resistivity leading to reconnection. This assertion is proven wrong, for example, by MHD simulations conducted by *Birn and Hesse* [2000], who show an ideal MHD simulation of a magnetic X geometry that remains stable for the lack of resistivity or other nonideal MHD terms. Because mesh convergence, as defined by GPL99 and shown in their example, is not synonymous with convergence toward the true MHD solution, it can at best be a necessary condition for convergence. Consequently, GPL99's implication that mesh convergence proves the correctness of their results is wrong.

Third, I am somewhat surprised that GPL99 do not show results from their model with added resistivity as proposed by R99. This test is easy to apply and would be more revealing because it gives a numerical estimate of the inherent numerical diffusion. GPL99 acknowledge that such tests are important and should be done, and we would agree that they would certainly be illuminating.

Fourth, I have done mesh-convergence tests in the past with my code, and I am convinced that the results presented in R99 are well converged to the extent required by the R99 study. In particular, I have measured the convergence rate and found it consistent with second-order truncation errors. I should note, however, that mesh convergence may pose a problem (for any code) when southward IMF cases are considered. In that case the tail current sheet collapses to one or two grid cells regardless of resolution, unless it is kept broader by resistivity. Since the simulations of R99 address northward IMF situations there is no concern in these cases. I also note that the simulations of R99 employed 933,120 grid cells with a resolution of roughly 0.5 R_E in the regions of interest. This is not much different from the simulations reported by GPL99, considering that the simulations presented by GPL99 employ the highest resolution in the near-Earth regime.

4. Bow Shock Diffusion

GPL99 also mention an effect seen in the R99 simulations that they describe as diffusion of the bow shock. This effect only appears in the cases of high resistivity. I had seen this effect previously but did not find it worthy of explaining in the paper. The "diffusion of the bow shock" is actually diffusion of the magnetic field components from the magnetosphere into the solar wind. Because the resistivity makes Faraday's law parabolic (as opposed to hyperbolic in the ideal MHD case), the magnetospheric field can penetrate upstream into the solar wind. Eventually, this leads to a strong modification of the IMF and solar wind, such that for northward IMF the IMF field lines (which should be straight south-north) bend away from the magnetosphere and for southward IMF they bend toward the magnetosphere. Of course, as mentioned by R99, numerical resistivity is nonuniform. In particular, flux-limited codes should have extremely small numerical resistivity in the region of uniform solar wind flow and IMF because there are no gradients. Therefore this effect is expected to be observed for uniform resistivity, which R99 choose for simplicity, but unlikely to be seen in codes that use flux limiters. This conclusion is supported by the test case presented in Figure 1, which has large numerical resistivity, yet shows no effects of "bow shock diffusion." For these reasons, GPL99's argument that the lack of this effect in other codes proves their low level of numerical resistivity is not correct.

5. So, Whence the Differences?

GPL99 state: "The mesh-convergence study shown in Figure 1 shows that decreasing numerical dissipation results in shorter closed magnetotails." With respect to what I have said above, I think it only shows that a finer grid in a particular model results in shorter magnetotails. GPL99 call the differences that I find between my first- and second-order runs puzzling. I do not find them puzzling at all, because there is a sound physical explanation for these, which is given by R99 (diffusion of lobe field across the tail neutral sheet and decoupling of the field line motion from the plasma flow). On the other hand, I find it rather puzzling how their first- and second-order models compare. I cannot think of a physical reason that would explain why more resistivity in a code should produce a longer magnetotail, let alone an open one, and GPL99 make no attempt to explain. However, their simulation of the transient response to a northward turning of the IMF (Figure 3 of GPL99) reveals inconsistency with available data. In their model it takes $\sim$52 minutes for the IMF to reach the magnetopause from the upstream boundary (178 R_E at 3.76 R_E/min to the bow shock and $\sim$5 min from the bow shock to the magnetopause). At t=105 min (i.e., 53 min later) the magnetosphere has completely closed (Figure 3 of GPL99, 105 min); possibly already at t=90 min (i.e., 38 min after the northward turning of the IMF). While it may be difficult to prove with data that the magnetosphere is closed at a particular instant, the closure rates of the polar cap for northward IMF were determined by *Newell et al.* [1997], who find that the closure of the polar cap takes at least 4 hours. This differs by about a factor of 5 from the GPL99 result, indicating that the processes that dominate this simulation are quite different from what happens in the magnetosphere.

R99 states that large resistivity values are sufficient to produce a closing tail. This statement implies that large resistivity values are not a necessary cause and that other causes are possible. GPL99 list some of the causes which they think might be important. None of these are investigated in any detail, but most are unlikely:

5.1. Incomplete Convergence to a Steady State

I find it questionable whether there is ever a steady state for the magnetosphere. Nonetheless, 6 hours of constant northward IMF represent several Alfvén transit times for the whole magnetosphere system. Thus, if there is a steady state, it should have been reached to a good approximation.

5.2. Physical Boundary Conditions and Their Numerical Implementation

Most models, including models 5 and 7, put the outer boundaries well into regions of supermagnetosonic flow; thus little if any effect on the solutions is expected. Ionospheric boundary conditions have not been investigated, but models 1-3 differ notably from the rest in that they essentially have no well-defined ionospheric boundary, yet they still produce closed magnetotails. Initial conditions may cause significant differences because an initially closed magnetosphere should never become open under due northward IMF. While GPL99 apparently start their simulation with a southward IMF, several models of Table 1 in GPL99 make no statement about their initial conditions.

5.3. Anomalous Resistivity

R99 clearly states that the anomalous resistivity term has no effect in the simulations. This becomes clear by comparison of run A with run B of R99. Run A contains the anomalous resistivity term, while run B has no anomalous resistivity but has uniform resistivity whose value is apparently well below inherent numerical resistivity. There are essentially no differences.

5.4. Nonuniqueness

I agree with GPL99 that mathematically nonunique solutions are unlikely. However, this would be hard to prove.

6. How to Resolve the Differences?

GPL99 suggest that "careful comparisons of the various models on several simple benchmark cases" should be carried out. I dare to disagree. The only relevant benchmark is the magnetosphere itself which we pursue to understand. Because there are no sufficiently complex MHD problems with analytic solutions available that could serve as comprehensive test cases, such intermodel comparisons would most likely only add confusion. In situ data that are suitable as model input and for comparison with model output are widely available through the International Solar Terrestrial Physics (ISTP) program and other sources. Model event studies [*Fedder et al.*, 1998; *Pulkkinen et al.*, 1998; *Goodrich et al.*, 1998; *Elsen et al.*, 1998; *Slinker et al.*, 1995; *Frank et al.*, 1995; *Raeder et al.*, 1997, 1998; *Berchem et al.*, 1998a, b; *Ashour-Abdalla et al.*, 1998] have so far added much more to our knowledge of the magnetosphere and its processes than any study that relied on modeling alone and appear much more worthy of pursuing.

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